

## OPEN LETTERS.

### The nomenclature question: theoretical objections to a stable nomenclature.

In the August issue of the GAZETTE Dr. B. L. Robinson by a skillful argument endeavors to demonstrate a certain alleged weakness in the Botanical Club principles of nomenclature. In this case, however, as in all matters concerned with nomenclature discussions, we should, as Dr. Robinson has more than once insisted, deal with actual illustrations, not with theoretical objections. In the matter of Otto Kuntze's non-application of the principle known as the rejection of homonyms to his genus *Aster*, I stand corrected. Such correction, however, invalidates this particular case of the support it would otherwise offer to Dr. Robinson's objection to the principle in question, for he still occupies his original position of citing only suppositious cases. The validity of this objection, it must be pointed out again, rests not on what *might* happen but on what *has happened*. The Botanical Club list enumerates about four thousand species, to all of which the principle has been applied, and if it contains any cases approaching in absurdity those Dr. Robinson has held up as bug-bears, I do not know of them.

I accept with pleasure Dr. Robinson's explanation of his reference to the representative character of the Madison assemblage of botanists, and in my turn I must explain that my interpretation of his remarks on that topic was due to the fact that the alternative explanation, the one he now presents, seemed impossible of maintenance. The records will certainly show that the principles themselves were formally adopted by the Botanical Club. The committee was then instructed to prepare a list in accordance with these principles, and this they have done. As for making the list itself "official" by some kind of formal vote to that effect, I am inclined to think that I for one should oppose such action. Surely the list must be supported primarily, and it may perhaps be said exclusively, by its conformity to principle. Any errors it contains cannot be made correct by a mere vote, nor, if an error is demonstrated, can one be expected to go on repeating it.

Probably the greatest objections that can be urged against the Association principles of nomenclature are those which may be brought forward relative to this very rejection of homonyms. These objections Dr. Robinson alone, among all the opponents of a rational nomenclature, seems to have grasped. I may perhaps be able to render some assistance in the discussion and elucidation of the subject.

The vital reason for the rejection of homonyms may be expressed as *the rejection of revertible names*. If a *Juncus megacephalus* of Curtis, 1834, has been for insufficient reasons relegated to synonymy and it is now an open question whether the species may not be a valid one, surely no one, with a view to stability, can oppose the rejection of a *Juncus megacephalus* of Wood, 1865. The latter name being replaced

by another, it matters not from a nomenclature standpoint whether the *J. megacephalus* of 1834 be treated as distinct or as identical with some older species. To go a step further: a variety *paniculatus* of Engelm., 1868, must be erected into a species. The name *Juncus paniculatus* is already twice preoccupied by European plants, both now referred to other species. Who is to decide whether either of these is likely to prove valid? Are not different decisions likely to be rendered by equally reputable botanists, whose conceptions of these species may differ? The committee has answered these questions, as well as all the other questions which logically follow them, by saying, "We will reject not merely those homonyms which we know to be revertible and those which we suppose may perhaps be revertible, but we will reject all homonyms and thus make revertibility impossible." Considered from the standpoint of stability the wisdom of this decision is, it seems to me, incontrovertible. I am well aware, however, that the considerations I have just mentioned will have no weight with Dr. Robinson if he really believes, as I can hardly bring myself to think he does, that a stable nomenclature is impossible. If having reviewed the whole Botanical Club principles adversely and with critical care, he finds in them only one possible chance of instability, and if from a list of four thousand species he does not cite a single case in point, one surely cannot ask for a more favorable commentary on the stability-producing capacity of the system.—FREDERICK V. COVILLE, Washington, D. C.

#### Decapitalization.

Mr. Sheldon's open letter in the June number of the GAZETTE affords me another opportunity to speak against the tendency of many botanists to follow a bad example set by our friends the zoologists: that of decapitalization of specific names derived from proper names.

If Mr. Sheldon writes his specific name *bajaensis* then Prof. Greene is right in his claim that it is a trivial and meaningless combination of Spanish and Latin. *Bajaensis* on the contrary leads us to the knowledge that the species came from Baja, a town in Mexico. His co-reference to *nevadensis* as a good Latin specific name is another instance. *Nevadensis* claims the species to have been found in the state of Nevada, while *nevadensis* leads us to the belief that the species is of the whiteness of snow, and would be at the same time again a Latino-Spanish "jumble."—C. F. MILLSPAUGH, Field Columbian Museum, Chicago.